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was from west to east; that they were emigrants from European lands; and, in fact, were none other than a part of our old friends, the Pelasgi of Greece; and these in turn were of the same stock as the Tyrseni and Etrusci of Italy. Sufficient can be said for the theory to make it worth further and serious investigation.

An older theory, to wit, that the true Hittite is an Indo-European language most akin to the modern Armenian and probably its ancestor, has been revived with considerable force by Professor Peter Jensen of the University of Marburg. His article is in the *Sunday-School Times*, March 25 and April 1. His criticism on Peiser's theory, that it is allied to the Turkish, is severe and merited. With reference to the puzzling and complex questions suggested by the inscriptions and ethnic types presented by the ancient monuments of Cilicia, he proposes the theory that the rulers of this district were at one time Semites or strongly semitized, while the mass of the population was of Indo-European blood. His opinion of his predecessors' studies is briefly summed up in these words, with reference to those of Sayce, Conder, Peiser, Ball, and Wright: "All are without foundation, and their results are destitute of value"!

A Linguistic Map of Guatemala.

Dr. Karl Sapper of Coban, Guatemala, has published in the first number of *Petermann's Mittheilungen* for the current year a map showing the present distribution of the native languages in Guatemala, accompanying it with a carefully prepared article on the dialects and culture conditions of the descendants of the aborigines of that country. Much of it is from his own studies, much of it from the excellent works of Dr. Stoll. He does not seem to be aware of the publication by me of the material collected by Dr. Berendt on the Xinka, the Pipil of Acasaguastlan, and other dialects. He falls into the rather serious error, which I pointed out in a paper published by the Congress of Americanists (session of 1890), of locating a language of the Mixe group in Guatemala, though he adds that no signs of it now exist. It never was there. He fails to solve the only real obscurity which remains in the linguistics of Guatemala, that is, the identification of the Popoluca located by the historian Juarros at Conguaco, in the partido of Guazacapan, which was not Xinka.

The language of Yupiltepec he considers a dialect of the Zinca, and brings into closer relationship the Chorti and the Chol. His expressions about the dialect of the Cajaboneros are not clear; in one sentence he speaks of their tongue as containing elements fundamentally diverse, "ursprünglich fremd," to the Kekchi; and in another refers to this element as perhaps Chol, which is merely another Maya dialect.

While Dr. Sapper's work is open to these slight criticisms, it is in the main worthy of the highest praise.

The Earliest Extension of the Iron Age.

In these notes (*Science*, March 10) I referred to some recent studies on the early Iron Age in central Europe. The question still remains, When and how did the art of working iron reach those localities? Two valuable papers of late publication have interesting suggestions touching this point. One is on "Le Premier Age du Fer au Caucase," by M. Ernest Chantre, who for twenty years has travelled, studied and excavated in the Caucasus; the other by M. Louis Siret, scarcely less distinguished for his archeological campaigns in Spain. Some remarkable coincidences are pointed out by both.

M. Chantre finds that the most ancient sepulchres in Lower Chaldea which contain iron are shown by their funerary contents to be contemporaneous with the third and fourth dynasties of Egypt, at which period occur the first signs of this industry on the Nile. At the lowest, this would place them 2500 years, B.C. The knowledge of the metal reached the southern and central vales of the Caucasus about 1500 B.C., through the extension of a "Semitic-Kushite" people, who were the ancestors of the modern Ossetes. They were distinctly non-Aryans, and the art of working iron was not introduced by them into Europe. Later on, about the seventh century, B.C., their culture was deeply

modified by invasions of Mongolic hordes from the East. (All this in spite of the fact that the modern Ossetes speak an Aryan tongue!)

The proof of this early Semitic influence is found in the identity of art-motives, decorations and methods, and especially in the numerous traces of the worship of the goddess Ishtar, the Astarte of the Phoenicians. In the Caucasus, as elsewhere, her favorite symbol, the dove, is constantly met with in ancient tombs; as is also that of the hand, employed in her rites as the symbol of adoration and peace.

It is true, as M. Chantre remarks, that in every station of the earliest iron age in Europe, from Greece to Scandinavia, we find figurines of birds, evidently sacred, and all to be traced to the dove of Astarte. They are proofs of what impressed M. Siret so much in his study of the earliest civilization of the Iberian Peninsula,— "the worship of a female deity represented under various symbols." He also, in his article in *L'Anthropologie*, 1892, No. 4, is forced by the results of his own excavations to assign this civilization to the daring early navigators of Semitic blood, to the Phoenicians, sailing from the far east of the Mediterranean, rounding the rocky shores of Spain in search of tin from the Cassiterides, or amber from the far-off shores of the Baltic. The first signs of iron there follow without a break on a highly developed bronze period; and its earliest discovered use was as rivets to fasten together plates of bronze. This indicates peaceful introduction and artistic growth, not the result of violence and conquest. The merchant, not the warrior, was the civilizer.

LETTERS TO THE EDITOR.

* * Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.

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Sham Biology.

THE article "On the Emergence of a Sham Biology in America" undoubtedly brought much joy to many botanists. Some of us know from experience that many American botanists are never so much tickled as when some one has gotten them to believe a zoölogist is hopping about a botanist and is worrying over the adhesive soil the botanist is trying to shake from his own trousers and boots onto those of the zoölogist.

Feeling himself above any and all of the charges made in the interesting tirade, the present writer has concluded he ought to at least make an attempt to show how strong a position his colleague had taken. No chuckling botanist can have any rational ground for gleefully pointing to me as a zoölogist badly hurt. Notwithstanding this, I am fully aware of the fact that nothing others may do or say is too minute to impel some people to strike their breasts, pour out eloquent prayers of thanks, and then go their way rejoicing over the capital they imagine can be made out of the sins of others. I also must say I am not at all sure of what my fellow-zoölogists will think of me for daring to answer for others. The unqualified and sweeping statements in several places technically include me, and this fact I offer as an excuse for attempting to indicate to botanists that the "sham biology" article is not so impregnable a piece of scientific work as I know many think it is.

It may be well to forestall possible taunting thrusts by stating that I have never desired to give a course in "general biology;" that I never attempted to plan, nor even thought of planning, a course in general biology to be given under my supervision, though the opportunity to do so was before me when I came to the University of Minnesota as an instructor. I have always insisted, and now insist, upon the independence, the autonomy of the two sub-departments of biology. My whole record stands as a proof of this, and therefore it cannot be said with justice that I belong to a class often called "sore-heads."

Let us at the outset agree to take the figures of speech for what they evidently were intended to illustrate, and not try to divert the real issues by seizing an opportunity to nag our immaculate brethren in botany.

We can all agree to the definition of biology. If it is to be improved at all, it must be made more general by saying it is the science of matter in the living condition. The writer tells us the word is "still to be defined as by Lamarck and Treviranus—both distinguished botanists." Will the botanists observe the innocent way in which their extremely generous champion here puts forth a foreclosed first mortgage claim on these distinguished naturalists—*biologists*. And this, too, so imperturbably in a discussion whose whole tenor is to brand good zoölogists as out-and-out usurpers. Now couple the definition of biology and this innocent act with "I have not at present time to discuss the fundamental absurdity of courses in 'general biology'—as if it were possible to plunge boldly into comparative study of plants and animals before one has studied plants and animals themselves." Who is now sure of what the writer means by "biology" and "general biology?" Is general biology a bold plunge into the comparative study of plants and animals before one has studied plants and animals themselves? Perhaps it is. Perhaps it is not. I have yet to learn of the fool board of trustees that is paying some fool instructor attempting to compare things without considering the things being compared. Is a course in "general botany" a course in the comparative anatomy of plants? Is a course in "general biology" to be proper only when it brings the student to the level of the genius who has been studying plants and animals for thirty or more years? If it is, then away with your "fundamental absurdity" of long courses and short courses and three-month courses in botany—as if a man could plunge into comparative study of plants without studying plants themselves! The figure of following "analytical statics" "up by geometry and the calculus" is not at all to the point, and it indicates an altogether different conception of the term biology and the phrase "general biology" than one would be led to expect in view of the explanation previously given. It might be well for our iconoclast to consider the definition of biology above suggested, and to ask, What is a plant and what is an animal? I do not mean to imply that in biology we are not to call living things plants or animals and that the student is not to study the things under these names. At present it is impracticable to do otherwise. But I do mean to imply that it is possible to teach the general laws and principles of biology in connection with the study of both plants and animals themselves. Moreover, I maintain that it is beyond rational objection that the student who studies well a *Pteris* and a *Lumbricus* (I use the words to indicate centres) has a broader and better foundation of facts for the great generalizations of biology than the student who studies only a *Pteris* and a *Ranunculus*. We will admit without discussion that the end to be attained by a general course is both informational and disciplinary. Now I cannot admit that the methods of study in botany are inherently different from those of zoölogy. Botany and zoölogy are coördinates of biology, and the methods in either must be biological methods. So far as the discipline is concerned, therefore, a term devoted to the study of plants and one to the study of animals will give as good results as two terms devoted to the study of plants, and certainly more information of value in every-day life will be gained in the former course. I cannot see why this could not be true even if sixteen full weeks, for example, were given up to the study of animals and only eight to plants. (The botanists have my permission to exchange the number of weeks.)

After all, it is not evident to me that the "fundamental absurdity" has caused this spasmodic cough of the botanist. The thorn seems rather to be—some zoölogists are conducting courses in "general biology," and, naturally enough, ultimately guide the more interested students into zoölogical lines of work. The botanists have confessed that they can't conduct such courses because they become so one-sided as to be unable to see that there is anything good in zoölogy they can't duplicate in botany—they become, if I may venture a figure, soles with their eyes on the under side, and then they croak about "fundamental absurdities." Is jealousy loose among our botanists? It is safe to say if the botanists had this monopoly of courses in general biology they would not talk about "fundamental absurdities."

It seems to me that the paragraph containing the words "funda-

mental absurdity" is full of extravagant utterance. Is it not absurd to say a zoölogist can't write anything worth reading upon the anatomy of *Pteris*? Is it not absurd to say only specialists can write anything of value? Either such statements are wild and not sufficiently guarded or the botanists are all fools. And even fools sometimes write things worth quoting. Certainly the zoölogists do not at present believe the work of all botanists is unreliable and should never be referred to.

I now come to the specific charges. Without giving any reasons, I must beg to differ entirely with the writer as to the question of the phylogeny and ontogeny of the "sham biology."

The matter of the inadvertent use of the term biology is, of course, to be regretted. But it ought to mollify the iconoclast somewhat to see that the nomenclature in our university organization is nowhere logical and consistent throughout.

I am not sure that I know just what is meant by saying Johns Hopkins University would have a better influence with "an honest naming of the zoölogical courses that *were* provided for." I was not aware they had ever been named otherwise. Certainly the zoölogical courses were named in accordance with their subject matter when I was at Johns Hopkins University. And I can assure the writer it will take more than brilliant rhetoric and insinuations to make me believe conditions have so changed as to have necessitated a dishonest naming of the zoölogical courses. I cannot speak positively about the present course in general biology at this noble university, and will therefore speak only of what existed several years ago. The course in general biology, as I got it, was by no means as one-sided as any botanist would have made it. A general biology course that will develop enough love for botany in a student to make the purchase of such expensive books as Sachs and various monographs a pleasure, and that will develop a respect for botany and admiration of botanists and their magnificent work, such as I know has been developed at Johns Hopkins, should hardly be dubbed "sham biology," simply because zoölogists were in power. If the facts gotten were wrong, it was the botanist's own fault; for the references were to the recognized authorities. If the course leaned any way in my case, it was toward botany. I have heard it said the course took up two or three plants and a dozen or more animals—that a few weeks were given to plants and months to animals. That certainly is not a true picture of the course I was privileged to get. And why must a course, in order to be above the shadow of a "sham biology," consider just as many plants as animals? Must just as much time be devoted to plants as to animals? It seems to me no weaker principle could be adopted. If the botanist directs such a course of study, he will naturally illustrate more with plants. The zoölogist will naturally use animals more frequently. But this does not necessarily produce a sham biology. I admit a decided zoölogist or a decided botanist will always be in danger of curtailing the sister-science too much, and a course laid out by the one or the other may, naturally enough, not be altogether satisfactory to the colleague "not in it." Such facts do not touch the possibility of botanist and zoölogist conjointly formulating a course in general biology. If the principles, laws, and generalizations to be impressed upon the student be taken as the guide, and the two kingdoms of living things be viewed as the store-houses of facts, a true general biology becomes a possibility. Why should a school of biology, organized with a professor of zoölogy at its head be any more a school of sham biology than a university with an ichthyologist as president be a sham university?

The "always insular capabilities of the Johns Hopkins biologist for blatant philistinism in regard to things botanical" would be an unpardonable fling in view of what men the writer's previous statements would make it cover were it not for the fact that it appears to have been written with the ghost of that pamphlet (which I had supposed dead, because of its absolute flatness) dancing before him in "cool effrontery." But even the pachydermatous zoölogists can appreciate moderation; and it is no weakness to keep one's just appreciation of an evil that does exist in some places under the influence of reason.

There are many things in the way of criticism and explanation yet to be said, but I will close by pointing out what an influence

botany seems to be having on my estimable colleague. Some zoologists divide the organs of animals into the vegetative organs, their functions being those common to plants and animals, and the organs of animal function, their functions being characteristic of animals. My genial associate must have learned this fact from some one and makes a desperate effort to use it in classifying the sub-sciences of biology by trying to limit zoölogy to the vegetative organs of animals, and relegating the animal functions to psychology, which is held "coördinate with zoölogy rather than as one of its sub-divisions." I may be wrong, but that effort looks like a bid for a vote.

To guard against any misapprehension on the part of those not acquainted with the actual attitude of the departments of botany and animal biology toward each other at the University of Minnesota, I must say that Professor MacMillan and myself are not at loggerheads here, but that we do and always have pulled together for the equal advancement of both botany and animal biology. The adjustment of our courses is not the result of a compromise, but the individual and united recognition of facts and conditions. We are not competitors, and there is no likelihood that we shall become such.

HENRY F. NACHTRIEB.

Professor of Animal Biology, University of Minnesota.
April 18.

On Methods of Defending the Existence of a Sham Biology in America.

Two recent papers in *Science* deserve a little attention at this time, for they serve as examples of the kaleidoscopic movements by which "biologists" hope to defend themselves against the clearly stated charges of incorrect use of terminology which have been brought against them. It will not be permitted to these wanderers from the path of orthographic rectitude to conceal their retreat under cover of a sea of ink. The discretion, good taste, enthusiasm of the writer, are not the subjects of the discussion and will not be discussed by him. No shuffling to alien positions can be admitted as an answer to the definite impeachment which has been brought against courses in zoölogy masquerading under the erroneous name of biology.

Although the briefer, the article by Mr. H. F. Osborn¹ of Columbia College should, from the acknowledged ability of its writer and its air of gentlemanly candor, be given first consideration. Mr. Osborn is under such manifest misapprehension, however, that it will be necessary first of all to correct him and indicate to him just the point at issue. He says "the arrangement of courses in Columbia is cited by Mr. MacMillan as a leading example of the manner in which botany is subordinated to zoölogy." Since absolutely nothing was said in my article about the subordination of botany to zoölogy at Columbia or anywhere else, I am naturally interested to learn by what higher criticism, textual or literary, Mr. Osborn has arrived at such an unexpected result. In my former paper it is written, "At Columbia College it is apparent that the subject of botany, since it stands by itself under its own organization, is supposed at least by the 'biologists' of that institution to be quite without the pale of their own science." It is my evident and distinct purpose here to charge, not subordination, but misuse of terminology. Indeed, if there were any "subordination" at Columbia, I should think it would be of the zoölogical courses staggering as they are under the weight of a false nomenclature.

In his note, Mr. Osborn cites a number of botanical and zoölogical courses at Columbia and then uses the word "biological" correctly in the sentence, "It does not appear that botany is ignored in this programme of biological courses in this institution." Immediately afterward he uses the word incorrectly when he says, "the fact that the botanical courses are not arranged under the *Biological Department* is a mere technicality of administration." A "Biological Department" without botanical courses is, however, something more than a "technicality"; it is a sham. Mr. Osborn is, of course, at liberty to have his department separated as he will; it is no affair of ours,—but why should he permit such a line as this from the circular of information,²

"Biology (Zoölogy) . . . Professor Osborn"? Why does he appear as defining the word biology as zoölogy? I am sure it must be for some better reason than the anxiety to use a high-sounding word, even though that word be used incorrectly.

Having thus indicated to Mr. Osborn the errors into which a probably hasty perusal of my former article has led him, I may now note his principal defensive movement. He says, "Biology, however, is not the science of animals and of plants, as Mr. MacMillan maintains, it is rather the science of life." Therefore, "those who set forth the fundamental principles of life are biologists,"—a fair paraphrase, I trust, of Mr. Osborn's argument. This is so unexpected a point of view to be taken by one of the leading animal morphologists of America that it is indeed difficult to collect one's self for a reply. The venerable style of talk about "life," I supposed, was extinct in scientific circles, unless one includes the metaphysicians. "Life," I had supposed, was an abstraction from certain observed phenomena of a group of things known as plants and animals. I presume Mr. Osborn does not use the word as does the Boston University in its Year Book,³ where Group IX. in Courses of Instruction is "Chemistry, Biology, and Geology," and Group X. is "Life, Personal Development, and Expression." I did not suppose that the statement that "biology was the science of living things" could possibly find objection in such a quarter as Columbia College. Here at Minnesota we are busily studying living things, but if Mr. Osborn is studying "life," he is evidently on another plane altogether. Long ago, one used to hear of "vital force" and "life," but I supposed we now believed that the best way to learn about life was to study living things. If it is true that the zoölogists are going in for the study of "life" under the belief that biology is not the science of living things, I wish them God-speed on a perilous, if ancient, voyage. And if this really is the modern view of "biology," I yield me a captive to Mr. Osborn's convincing argument and beg to withdraw among those botanists who believe that botany is the science of the living things, plants, and will certainly, if I know them, be glad to leave the study of "life" open to the zoölogist—"biologist," who rules out living things as irrelevant to his science.

Let me, in closing, call the attention of Mr. Osborn to the fact that I am unaware of any one-sided state of true biological education in America. There is nothing one-sided about it in Harvard University. It is the sham biology that is one-sided, and for this the zoölogists are responsible in large measure, therefore the epistle is addressed to them. I recall now but one institution which names its botanical courses, a "department of biology." And this department is manned by a Johns Hopkins doctor of philosophy, from whom one might unfortunately expect the one-sided view.

The paper by Mr. Francis H. Herrick,⁴ entitled "On the Teaching of Biology," requires some elucidation and correction that I may venture to give. Notwithstanding its characterization of my former article as "thoroughly bad," I take pleasure in acknowledging its own uncommon excellence. Any defense of the sham biology is sufficiently difficult, and while the air of righteous enthusiasm was accurately enough predicted it was scarcely realized with what vigor the plaintiff's attorney would be afforded the treatment sanctioned in such cases by all the traditions of the bar.

Aside from its entertaining personal character, the contribution by Mr. Herrick appears to seek the establishment of the following points: (1) The study of biology is not two disciplines, but one discipline; (2) biological science is not to be set over against physical science, but is to be included in it; (3) zoölogy, when presented under the name of biology, is not a sham biology, but a "restricted biology"; (4) the better fundamental division of biology is into general morphology and general physiology, not into botany and zoölogy. Stated thus, with such condensation as is necessary for clearness, it is hoped that the exact meaning of Mr. Herrick is preserved. These four points, only the third of which seems to have direct bearing on the question at issue, may now receive their proper attention.

¹ Science, Vol. XXI., p. 234. New York.

² Columbia College Circular of Information, 1893, Pt. VII., p. 4. New York.

³ Boston University Year Book, Vol. XX., p. 66, 1893. Boston.

⁴ Science, Vol. XXI., p. 220. New York.